A

DEFENCE

OF THE

OBSERVATIONS

ON THE

FIRST CHAPTER

OF A

BOOK

CALLED

MISCELLANEA ANALYTICA.

By Dr. Powell.

LONDON,
Printed for T. MERRIL, at *Cambridge*. MDCCLX.

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THE same concern for the honour of the University, which prevailed on a person, who has employment enough of other kinds, to bestow some hours in examining part of a book, called *Miscellanea Analytica*, and to lay his observations before the University, whilst the Writer was a candidate for the mathematical professorship; would have inclined him to be silent after the election. He would have been content, that the new Professor should be thought by all, who either do not attend to, or do not understand the subject, to have gained a complete victory; had the Professor been content with a full display of his own strength, and a proper scorn of his antagonist; and had not, a little inconsistently it may seem, added complaints to contempt; crying out of one, whom he tramples beneath his feet, as *unkind, severe* and *cruel*. These are accusations, to which *the Observer* cannot willingly submit. He finds himself therefore obliged to defend his remarks. But he will do it as tenderly as possible. He will make no addition to his list of mistakes be-

fore produced; and is ſorry, that he cannot make it ſhorter.

The Profeſſor thinks the *reflections upon the obſcurity and perplexity of his manner of writing very ſevere and cruel*; becauſe his *view in publiſhing his work, was not to ſhew how well* he *can write.* Nor was it the deſign of the obſervations to ſhew, how ill he writes, by inſtances of falſe grammar, unuſual expreſſions, or perplexed periods; but by ſuch faults only, as totally conceal from us, what he certainly intended to make known, the ſucceſs, with which he has beſtowed his *time and labour in the ſtudy of this ſcience.*

The privileges, which the Profeſſor claims under this title of a bad writer, are very numerous and extenſive. He uſes ſo many *peculiar facilities* in his reaſoning, that it is indeed difficult to diſcover, by which of all the common principles of his ſcience, he will allow us to judge of his work. If in the ſame algebraical proceſs we meet with the letter *n* uſed twenty times; we are not to ſuppoſe, that it has every time the ſame meaning. No, he leaves it to the reader's ſagacity to diſcern, that it ſtands eight or ten times for one number, and as often for another.—If in a computation, he ſubſtitutes one quantity or expreſſion for another; we muſt not from hence infer, that he thinks them equal.—If we find + put for — and — for + not once only, but many times in the ſame problem; we muſt ſuppoſe, that the diſ-

tinction between these signs is a matter of fashion, rather than of necessity.—If, after the allowance of — for + as often as he pleases, the calculation does not appear to be right; we must suppose, that the notation is unusual.—Each of these is, I think, the reply he makes to some one of my remarks. And I mention them only to excuse myself, if I say but little in support of objections thus answered. In these questions, the matter seems to be brought to an issue. In the rest, I will argue with the Professor, on such principles, as he has not yet expressly denied, that 4 is greater than 3, and that 1 + 4 is equal to 5.

But has not the Professor hurt his own title to the privileges above mentioned? Or has it been done for him by a friend? One perhaps, who knew *enough* of *law* to tell him, that in a free state, as is the common-wealth of letters, *privileges are odious.* For whatever may be thought of the *Miscellanea Analytica*, the *Reply* without doubt, comes from a *good* writer. When he has any thing to allege, he expresses it clearly. When he has nothing, he supplies the defect with an handsome air of contempt and confidence. The principal parts of the objections he conveys away, and slips into their places some trifles of his own, with so much dexterity; that he appears to be one, who understands, as they say, *the whole game.* It will be necessary for me to call upon him to play above board; and I shall do it by stating in few words my remarks

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on each proposition, and considering his answers to them distinctly.

The observations on the first lemma were these:

1. That it is not demonstrated;

2. That it is not shewn to be true in any one example;

3. That in many examples it is known to be false;

4. That the Writer was led into this error by misunderstanding a passage in Dr. *Saunderson's Algebra.*

To the first observation his reply is, that *there are five steps in* his *demonstration, and* that he *knows not how to put in more.* I had called it only one step. He minces it into five. Let it be five, if he pleases. In four of them a child could follow, or rather lead him. But one is a Giant's stride. Here is the difficulty, in which the Professor leaves us. He asserts, that *the equation, which reduces another to* m *powers, finds all the combinations of* m *of its roots.* How are we to be convinced of this proposition? Surely it is not self evident. And we have the Writer's acknowledgment, that he knows not how to prove it. For that would be to put more steps into his demonstration. And yet this single point contains the very essence of the pro-

problem. All the reſt without it is inſignificant.

Thus the firſt queſtion between us is made very ſhort and clear. That, which he calls a demonſtration, appears to be a mere *petitio principii*. If therefore it ſhould be ſaid again, that not one of theſe objections *has even the appearance of truth*; we may expect to be plainly told, whether the propoſition before cited ought to have been admitted as an axiom, or has been demonſtrated by any other Writer.

Or, if the Profeſſor is unwilling to anſwer this queſtion, let him take another method. Let him conſider the lemma again, and again; let him make what uſe he pleaſes of the proof, which, he ſays, Dr. *Saunderſon* has given of it in a ſpecial caſe; let him beg the aſſiſtance of all his friends, (and we may gueſs from the *Reply*, that he has one friend able to aſſiſt him;) if after all, he only gives us a clear demonſtration, drawn out in form and method, of this ſingle propoſition, "the equation which reduces "another to *m* powers, has, for its roots, all the "combinations of *m* roots of the former;" I accept it as a full anſwer to all my objections.

But till our Author is furniſhed with ſuch a demonſtration, ſhould he not have given us that lower kind of proof, which may be formed by the application of a rule to ſome few examples? One only is mentioned in his book; and, though the lemma is very extenſive, no other is added

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in the *Reply*. This one was called in queſtion: And the Profeſſor determines to rely on Dr. *Saunderſon* for the ſupport of it. But, in which of Dr. *Saunderſon's* words, we are to look for the proof here alleged, it is impoſſible to conjecture. If there be any ſuch proof of the neceſſity of uſing an equation of *ſix dimenſions* in the reduction of a *biquadratic*, it certainly ſlipt from him without his knowledge. For the title of his diſcourſe is *of the reſolution of all ſorts of* BIQUADRATIC *equations by the mediation of* CUBICS; and, having explained the reaſon of *Des Cartes's* method, he concludes thus, *hence ariſes the neceſſity of the intervention of a* CUBIC *equation in the reſolution of a biquadratic into two quadratics.*

But tho' the Writer will not conſider *Des Cartes's* rule to defend his own propoſition, he will do it to find fault with my expreſſion. He tells Us, that *Des Cartes in order to reduce a biquadratic equation to a quadratic makes uſe of an equation of ſix dimenſions of this form*;

$$x^6 - px^4 + qx^2 - r = o.$$

And then by the quadratic equation $x^2 = z$ he reduces this incomplete bicubic to a complete cubic: not obſerving, how plainly he here contradicts his own rule; according to which, if we would reduce an equation from ſix dimenſions to three, we muſt apply to it one, not of *two*, but of TWENTY dimenſions.

But

But this contradiction the Writer might have avoided, if he had liked another better. For he might have ſaid, that the two equations

$$x^6 - p x^4 + q x^2 - r = 0$$

and $z^3 - p z^2 + q z - r = 0$ are both cubics; in one of which the roots are conſidered as ſquares, in the other not. But then he muſt have acknowledged, that in *Des Cartes's* method a cubic equation is ſufficient. Let him take either ſuppoſition. They are equally inconſiſtent with his lemma. The difference is only in names; concerning which I am no more inclined to diſpute, than to anſwer the objection to my words, which he enlarges upon here, and returns to again in the 23ᵈ page, and which amounts exactly to thus much, that $+3$ and -3 ought to be called two numbers, not one number with it's ſign changed.

But it was obſerved farther, that this rule is known in many examples to be falſe. My firſt inſtance was the moſt obvious one; the caſe, where we know ſome of the ſimple equations, out of which the equation to be reduced is formed. In this the Profeſſor *can hardly believe* me *ſerious*; becauſe *the equation muſt be firſt ſolved, before this method can be put in practiſe.* He ſhould have ſaid, in part ſolved. But what if it had required a complete ſolution of the equation? It is not to the purpoſe to conſider, in what caſes this method can be uſed; but whether it is juſt, and contrary to the rule here delivered.

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Otherwiſe we muſt ſuppoſe, that the truth of the lemma depends on the ſkill of the Analyſt, who applies it. And this indeed will be a new diſcovery in Mathematics; a propoſition, which ceaſes to be true, when it ceaſes to be uſeful.

My other inſtances were taken from his own book, where equations are reduced by the help of others, which, unleſs I miſtake in counting the combinations of 3 in 5 or 6, have not the number of dimenſions required by this lemma. Nor is there, in the *Reply*, any attempt to ſhew, that they are conſiſtent with it.

My fourth obſervation did not require a particular anſwer. The Writer acknowledges on the one hand, that he depends on Dr. *Saunderſon* for the proof of this propoſition. And on the other I ſhall readily confeſs, that, if he can apply that proof to his purpoſe, he is an apt ſcholar, and has learned from his maſter more than the maſter knew.

But the Profeſſor concludes his reply to the obſervations on the firſt lemma with ſomething, that might perhaps be intended as an anſwer to them all together. *The Obſerver*, he ſays, *could not but underſtand, that my lemma relates to a general method: the particulars therefore, which he ſpecifies, are beſide the purpoſe, becauſe for particular caſes peculiar facilities may be invented, which will not extend to equations in general.* I had mentioned, as contrary to the lemma, not

only

nly the way of reducing *all particular* equa- ons, when some of the roots are found; but so the *general methods* of reducing equations of tain dimensions before any root is found; and d asserted, that there is *not any one* of them, which the rule here delivered is true. But is, it seems, is a method of methods; a way finding general rules for reducing all equa- ons; so comprehensive, as to take in every un- own case, but so confined as not to belong to y known one. Shall I beg leave to mention the Professor one of the plainest and most es- lished rules of logic, that a general affirmation contradicted by a particular negation? His eral proposition cannot be true, if it contains, t evidently does, the particular propositions, ich are acknowledged to be false.

All this is so evident, that it would lead one suspect, that the Writer of this lemma had a fused notion of something, which he has not ressed. If he will allow me to depart far n his words, I will even venture to guess, at his meaning should have been. Instead of quiring, what number of dimensions the equa- n *has*, by the help of which another is redu- d; he should have inquired, what is the *great-* number of dimensions, which in *any case* it *can* ve. If he will thus change his problem, the swer he has given cannot, as far as I know, be monstrated to be false. But it will still to- lly want both proof and use. He who would

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make the ſolution of an high equation eaſier, muſt ſhew Us, how to reduce it by another, which has fewer roots, than it has itſelf, not as many, or more.—Thus much of the firſt lemma.

My obſervations on the ſecond were;

1. That it is Sir *Iſaac Newtons's* ſolution expreſſed in a different form.

2. That the change of the expreſſion is ſuch as makes it leſs ſimple and conciſe.

3. That the change of the expreſſion is ſuch as makes it alſo leſs general.

4. That the Writer did not fully comprehend the change he had made.

To the two firſt of theſe remarks, the Profeſſor replies; *Eaſy as the Obſerver takes this* (to change the form of *Newton's* rule) *to be, it has exerciſed the beſt Heads of the laſt century.*—Demoivre *has many pages upon the ſubject. Even the immortal pen of* Newton *has been employed upon the ſame Problem; only changing the expreſſions, (as the Obſerver thinks) and deſtroying the original ſimplicity and conciſeneſs of the excellent rules, which he had received from his Maſters.* Inſtead of inquiring, whether we are obliged, as the Profeſſor here intimates, to ſpeak of Sir *Iſaac Newton* and of him in the ſame terms; or of comparing the ſolution of this problem with any thing done by Mr. *Demoivre*; I will only ſhew, by one ſhort

short example, how the series was formed; and leave it to the reader to judge, what sort of employment he has found for the best Heads of the last century. Let the equation be $x^n - px^{n-1} + qx^{n-2} - rx^{n-3} + sx^{n-4}$ &c. $= 0$ and suppose, that we would find the sum of the fourth powers of it's roots, then Sir *Isaac Newton's* rule is this; let $p = a$, $pp - 2q = b$, $pb - qa + 3r = c$, and the sum we are seeking will be $pc - qb + ra - 4s$. Now only substitute for c, b, and a the quantities before given as equal to them, and you have so much of our Author's series as belongs to this case. Do the same in three or four other cases, and the law of the series will be evident; far more evident, than it is made by the Author's account of it.

In this new rule no new idea is introduced. The whole mystery consists in merely substituting for single letters the several complex quantities, which each of them had been made to represent. And one would hope, that the Professor did not *bestow* much *time and labour* in this problem, as it is difficult to imagine, that it could give any man more trouble, than that of transcribing twice or thrice the terms of the series. The Professor indeed seems to be conscious, that it was not worthy of *Him*, though *it has exercised the best Heads* of the last century. For he adds; *If I have done nothing but change the expression of this rule, I have ill-bestowed my time and labour indeed. But the truth is, that I have solved another problem. Sir* Isaac Newton*'s rule teaches us how to find the sum of the roots of any*

any equation, the ſum of their ſquares, of their cubes, &c. one after another. My problem is to find the ſum of any propoſed power of the roots per ſaltum *without firſt finding the ſum of the inferior powers.* We can never deny our obligations to thoſe, who ſhorten for us the operations of arithmetic. To find the high powers of the roots of equations having many terms by Newton's rule, is perhaps ſomewhat tedious. A more compendious method cannot but be acceptable. Let us only examine it a little. The Profeſſor will not thank us for undeſerved praiſe. And that we may not deny him any that is his due, let us take an example from himſelf, and ſuch as will ſhew his improvement to the greateſt advantage; let us ſeek by each method the ſum of the 7th powers, the higheſt to which his ſeries is continued, of the roots of the higheſt equation we meet with in this chapter. The equation is,

$$x^6 - 4x^5 - 2x^4 + 20x^3 - 11x^2 - 16x + 12 = 0$$

And by Newton's rule,

the roots $a = p = \ldots\ldots\ldots$ 4
the ſquares $b = pp - 2q = \ldots\ldots\ldots$ 20
the cubes $c = pb - qa + 3r = \ldots\ldots$ 28
the 4th powers $d = pc - qb + ra - 4s =$ 116
the 5th $e = pd - qc + rb - sa + 5t =$ 244
the 6th $f = pe - qd + rc - sb + ta - 6v =$ 860
the 7th $= pf - qe + rd - sc + tb - va =$ 2188

This is the method of finding all the ſeven powers. The rules are ſhort, regular, and eaſily

ſily applied. But now ſuppoſe, that we want only the laſt, and that we would determine it *per Saltum*; then let us take the rule backward, and, without computing the inferior powers, begin with $-va = -vp = -48$, $+tb = tpp - 2tq = 320$, $-sc = -sp^3 + 3sqp - 3sr = 308$ &c. But this You will tell me is only a more tedious method, and of no uſe. On the contrary I ſhall inſiſt, as the Profeſſor teaches me to do, *that I have now ſolved a new problem; that* Sir Iſace Newton's *is a rule for finding the ſum of the roots of any equation, the ſum of their ſquares, of their cubes,* &c. *one after another; but that my problem is to find the ſum of any propoſed power of the roots, without firſt finding the ſum of the inferior powers.* And if you are not ſenſible of my merit, it is only becauſe I have laid my art too open. I ſhould have made theſe ſubſtitutions without explaining them; and then by adding together the terms, which differ in their coefficients only, I ſhould have arrived at the ſame ſeries which the Profeſſor, after the fruitleſs endeavours of the beſt Heads of the laſt century, has, with much time and labour diſcovered.—But let us apply it to the equation above.

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p^n . . . = ...	16384	
$-nqp^{n-2}$ = ...	14336	
$+nrp^{n-3}$ = ...		-35840
$-nsp^{n-4}$ = ...	4928	
$+n.\frac{n-3}{2}q^2p^{n-4}$ = ...	3584	
$+ntp^{n-5}$ = ...	1792	
$-n.\overline{n-4}\,qrp^{n-5}$ = ...		-13440
$-nvp^{n-6}$ = ...		-336
$+n.\overline{n-5}\,qsp^{n-6}$ = ...	1232	
$-n.\frac{n-5}{2}.\frac{n-4}{3}q^3p^{n-6}$ = . .	224	
$+n.\frac{n-5}{2}r^2p^{n-6}$ = ...	11200	
$-n.\overline{n-6}\,qtp^{n-7}$ = . . .	224	
$+n.\overline{n-5}.\frac{n-6}{2}q^2rp^{n-7}$ = .		-560
$-n.\overline{n-6}\,rsp^{n-7}$ = ...		-1540
	53904	-51716
	51716	
	2188	

We see then the whole meaning of finding the sum of any proposed power *per saltum*, so that we may continue ignorant of the inferior powers; and for this advantage we are taught to make a computation five or ten times more laborious, than the other.

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In one method, we keep diſtinctly in view the ſeveral lower powers, whilſt we compute from them the higher. In the other, it is equally neceſſary to collect the ſeveral products, which compoſe the lower powers; though they are ſo mixed together, that we cannot diſcern, what parts of theſe products belong to each power.

In his reply to the third remark on this propoſition, the Profeſſor has no patience with the *Obſerver's grievous miſtake*; who can offer no excuſe for himſelf, but his ignorance of the Profeſſor's peculiar notation, by which the ſame letter in the ſame proceſs is put for different numbers. He ſaw indeed, and mentioned, that it was eaſy to ſet it right; and he ought perhaps to have paſſed by this, as he did other privileged places, without too ſcrupulous an examination; and to have reſerved his attention for the third page, which contains, it ſeems, *a full ſynthetical demonſtration of the lemma*; *a peculiar ſpecies of demonſtration, and peculiarly adapted to the ſolution of this problem. It was firſt invented by* James Bernoulli : *he was a ſevere Mathematician, and never contented ſo long as he had left any doubt upon the mind of his Reader. He invented therefore this ſpecies of demonſtration for the proof of ſuch general propoſitions, as are derived by induction from particular caſes. And the obſerver has not been accuſtomed to this kind of demonſtration, though it be of great extent.*—And here I muſt fairly own, that I was entirely unacquainted with this invention of Mr. *Bernoulli*,

and not a little ashamed of my ignorance of so celebrated a discovery; never imagining, that the Professor, a plain modest man, as I am told, would have set forth all this parade about a mere nothing. But how was I surprized, when I looked into *Bernoulli!* How will the Professor be surprized, when he looks into *Euclid!* to find that this new, this curious, this peculiar species of demonstration, first invented by *James Bernoulli*, and just imported from *High Germany*, is no other, than we meet with many times in *Euclid's* Elements. Our great Mathematicians despise that easy old book, with which we, people of lower abilities, are content to begin our studies. But let me seriously recommend it to their perusal. To encourage them in the undertaking, I will produce to them, out of this forgotten Author, the very species of demonstration, with which they are so much delighted, and compare it with the original from Mr. *Bernoulli* himself.

The proposition, which Mr. *Bernoulli* * demonstrates by the method here commended, is this. The series of numbers following each other in their natural order, and beginning from o, is always equal to half the series of as many terms each equal to the last. And his demonstration begins thus. I suppose, says he, the matter to have been examined for some number of terms, and that having found it true to the term *a*, I there stop. Then he proves, that,

* See *Acta Erudit. Lips.* 1686. p. 360.

since

ſince it is true to a, it is true alſo to $a+1$, and for the ſame reaſon to $a+2$, and ſo on as far as you pleaſe.

In the ſame manner does *Euclid* demonſtrate ſeveral propoſitions in his ninth book. Part of the 8th propoſition will be a ſufficient example. If from unity numbers be taken how many ſoever in continued proportion, the third, and the following terms, intermitting always one, are ſquare numbers. The demonſtration firſt ſhews, from the 20th definition of the 17th book, that the third term is a ſquare number. But if the third be, then, by the 20th propoſition of the 8th book, the third from that, or the fifth is; and again the third from the fifth, or the ſeventh; and ſo on as far as you pleaſe.

Now the difference, in the forms of theſe demonſtrations, is no more than this. Mr. *Bernoulli*, though *a ſevere Mathematician*, aſſumes, as a thing ſufficiently manifeſt, that his propoſition is true in ſome ſmall number of terms. *Euclid*, a more ſevere Mathematician, aſſumes nothing, which is not contained in his definition, or before demonſtrated. And if Mr. *Bernoulli* is to be ſo much celebrated for this little departure from the rigid manner of *Euclid*; I ſhall claim the honour of a much nobler invention in the name of our Profeſſor, who, in his *full ſynthetical demonſtration*, aſſumes the truth of *Newton's* rule, the ſame in other expreſſions with his own.—

Whoever reads the propofitions in *Euclid*, or *Bernoulli*, will meet with nothing in this form of demonftration, that can perplex him. Whoever reads Mr. *Mac Laurin's* fhort comment on Sir *Ifaac Newton's* rule, will fee, that the fubject of this lemma can be made as eafy, as the common operations in *Algebra*. If he would alfo fee, how difficult one of the fimpleft forms of demonftration, and one of the eafieft fubjects may be made, let him return to this third page. He will find here fome of thofe *omiffions*, and thofe *unufual notations*, by which our Author delights to try the fagacity of his Reader, and which the Reader, when he difcovers them, will be apt to call miftakes.

An omiffion of a neceffary part of the law of this feries, and an obfcurity in another part gave occafion to my fourth remark. As to the obfcurity, the Profeffor replies, that the terms, having been explained (as I had obferved) by Mr. *Demoivre*, *are become notorious, and to have explained them again would not only have been unneceffary, but moft Readers would have thought it impertinent*. Whether words once explained by one Writer, become for ever after technical, is not to be determined by any arguments. If therefore the Reader can be perfuaded to confent to it, we will give up this point to the Profeffor. The principal part of the objection is, that fo much of the law is omitted, as will make every term, except three, of the feries determined by it, greater or lefs, than it ought to be. He anfwers,

wers, that, if the whole had been omitted, yet *the ſymbols would have ſhewn the law of the ſeries to a Reader of any practiſe and ſagacity.* We have the more reaſon to wonder, that they did not ſhew it to the Writer. This compliment to the ſkill of his Readers ſerves him for an anſwer to ſo many objections, that it were a pity to rob him of it. I will therefore paſs on.

When I took notice of this defect, I alſo intimated to him, how he might ſupply it. If he would only read again the page of *Demoivre*, from which he had been tranſcribing, he would find ſomething like what was here wanted. He is diſpleaſed at *an expreſſion of ſo little preciſion as this* ſomething like it. He has reaſon. For wherever in my little pamphet I told him fully, what his propoſition or concluſion ſhould have been, there his anſwer is always ready, that this is the very thing he meant. We ſhall meet with ſome remarkable inſtances of it preſently.— In the mean time we muſt proceed to the firſt corollary, on which I obſerved;

1. That the words, in which it is expreſſed, do not diſtinguiſh it from the lemma itſelf.

2. That it wants eaſe, and elegance, and uſe.

In ſupport of the firſt remark I ſhall content myſelf with tranſcribing the very words of the lemma and it's corallary.

LEMMA II.

Data æquatione, invenire ſummam radicum, ſummam quadratorum ex ſingulis radicibus, ſummam cuborum, ſummam quadrato-quadratorum & denique ſummam n *poteſtatum.*

COROLLARIUM I.

Ex hoc lemmate invenitur ſeries, quæ ſuumæ radicum, ſummæ quadratorum ex ſingulis radicibus, cuborum, biquadratorum, ... n *poteſtatum æqualis eſt.*

One might imagine perhaps, that the latter problem was to be anſwered by a ſeries, the other not. But this is not the difference. Our Author does nothing without a ſeries.

Whether the other remark on this Corollary be juſt or not, can only be determined by the application of the rule to particular examples. And the Writer would have replied to it properly, if he had taken two or three equations, and ſhewn us, that the computation is neater and eaſier by this ſeries, than by the rule out of which it was formed. As he has not, I will try it in a ſingle inſtance; the firſt we meet with in his own book.

$$x^4 - 8x^3 + 23x^2 - 28x + 12 = 0.$$

Suppoſe

Suppoſe that we want to know, the aggregate of it's roots, their ſquares and cubes; and any one acquainted with *Newton's* rule inſtantly ſees, without writing a Figure, that the ſum ſought is $8+18+44=70$. But if we follow this new method, in the ſearch of which almoſt all *the beſt Mathematicians that have lived within the laſt century, have been employed*; then we proceed thus: $\frac{8-8^4}{1-8} = \frac{-4088}{-7} = 584$. and -23. $\frac{2-8-4.\,8^2+3.\,8^3}{\overline{1-8}|^2} = -23.\,\frac{1274}{49} = -598$; and again $28.\,\frac{3-2.\,8-4.\,8+3.\,8^2}{\overline{1-8}|^2} = 28.\,\frac{147}{49} = 84$. But $584-598+84=70$. This is the ſhorteſt problem, to which the ſeries can be ſuppoſed to be applied. If we uſe it for any high powers, the labour increaſes much faſter than by the other method. And though the Profeſſor *engages to write down the anſwer as faſt as he can write with any care and attention*; it muſt nevertheleſs be owned, that he would ſadly waſt his ink and paper.

On the ſecond Corollary theſe obſervations were made:

1. That the anſwer to the problem ſhould have been confined to thoſe equations, of which every root is leſs than 1 and greater than -1.

2. That the rule here given contains this, among innumerable other absurdities, that $1 + 4 + 16 + 64$ &c. in infinitum $= \frac{1}{-3}$.

3. That the Writer was brought into this difficulty by not clearly understanding the doctrine of infinities.

To the first of these observations, the Professor answers, *that what the observer says of the limits of this problem, is true*; *but that most readers would have thought* him *a trifler*, *if* he *had mentioned them.* And *that it is not once in a thousand times that limitations of this nature are mentioned by the best writers.* How good soever these reasons may be, I must take the liberty to give a better reason, why this trifle was not mentioned; that it was not known to the Author, when he answered the problem. This may seem a bold assertion; but the proof of it is extremely evident. No man ever attempted a thing, which at the same instant, he knew to be impossible. No man ever undertook to determine a question, in which he clearly perceived a contradiction. It is not indeed uncommon with Mathematicians to propose problems, that admit no answers but within certain limits; and to give general determinations of them, which, if you apply them to any case not within those limits, become impossible. Such determinations are as extensive as we want; and can

can lead us into no error. When we purſue the problem into ſome impoſſible caſe, the general anſwer to it becomes not falſe, but unintelligible. But in the propoſition before us, we have an anſwer, which extends beyond the poſſible limits of the problem; and becomes not unintelligible, but falſe. It is therefore as certain, as any rule of reaſoning whatever, both that the demonſtration here given, is no demonſtration; and that the writer was ignorant of the limits, either of the queſtion, or of the anſwer.—The following are very eaſy examples of equations, whoſe roots are not all between 1 and —1, and yet the ſum of their powers in *infinitum*, is given by the rule here delivered. S ſhall denote that ſum.

$$xx - \frac{9}{2}x + 2 = 0 \quad S = -\frac{1}{3}$$

$$x^3 - 5x^2 + \frac{17}{4}x - 1 = 0. \quad S = \frac{2}{3}$$

$$x^4 - \frac{21}{4}x^3 + \frac{11}{2}x^2 - \frac{33}{16}x + \frac{1}{4} = 0. \quad S = 1.$$

$$x^3 + 2x^2 - \frac{11}{4}x + \frac{3}{4} = 0 \quad S, = \frac{5}{4}$$

$$x^3 - \frac{11}{2}x^2 + \frac{17}{2}x - 3 = 0 \quad S. = -\frac{5}{2}$$

A rule of this kind cannot be of any uſe even in the caſes that are poſſible. For either we know all the roots of the equation, to which we apply it or not. If we know them, and they are

are within the limits aſſigned, the rule is uſeleſs. If not, it is worſe than uſeleſs; it leads us into error. For there is an infinitely greater variety of caſes, where it is falſe than true.

Among all the wonderful things contained in the reply, there is none more ſtriking, than what the Profeſſor ſays to my next remark; that the equality of *the ſeries* $1 + 4 + 16 + 64 + 256 + \&c.$ to $-\frac{1}{3}$ is *not found by his rule*; that there are ſome *ſteps in the ſolution of the problem, which expreſs it*; and *that the obſerver read his book with ſo little attention, that it is very plain he has miſtaken theſe ſteps in the inveſtigation of the rule for the rule itſelf.* The Profeſſor forces me to depart a little from that reſpect, with which I am always inclined to treat him, when he makes it neceſſary in this diſpute on every occaſion to allege the very firſt principles of all reaſoning. It had been kinder in him to have recollected, without obliging me to remind him of it, that an error in the inveſtigation of a rule muſt be an error in the rule itſelf. It can never be otherwiſe, unleſs it be corrected by ſome oppoſite error; which in this inſtance has not *happened.*

But perhaps we ſhall not agree in theſe principles. Let us have recourſe to our Algebra: The examples in the laſt page were choſen on purpoſe to ſuit this caſe. The roots of the firſt equation are $\frac{1}{2}$ and 4; the ſeries of the powers

of

of the former, $\frac{1}{2} + \frac{1}{4} + \frac{1}{8} + \frac{1}{16}$ &c. $=$ 1, that of the latter is $4 + 16 + 64 + 256$, *&c.* but both together, or $1 + 4 + 16 + 64 + 256$, *&c. by the rule before us* are equal to $-\frac{1}{3}$. Now then let it be fairly ſaid, and without diſguiſe, whether *the obſerver miſtook a ſtep in the inveſtigation for the rule itſelf*, or the Writer did not at all apprehend what was contained in his own rule.—The roots of the ſecond equation are $\frac{1}{2}$, $\frac{1}{2}$, and 4, and the ſum of their powers in infinitum *by the rule before us* is $\frac{2}{3}$, or $1 - \frac{1}{3}$. Therefore as before $1 + 4 + 16 + 64 + 256$, &c. $= -\frac{1}{3}$. Again, the third equation has for its roots $\frac{1}{4}$, $\frac{1}{2}$, $\frac{1}{2}$, and 4. The ſum of the powers of the two firſt roots is $1 + \frac{1}{3}$. But the ſum of all the powers *by the rule before us* is 1. Therefore the ſum of the powers of the two laſt, or $1 + 4 + 16 + 64 + 256$, &c. is $-\frac{1}{3}$. Theſe examples will ſurely be ſufficient to convince both the Profeſſor, and his aſſiſtant in the *Reply*. And I may reaſonably hope, that they will never again aſſert, either that *the obſerver* did not underſtand the rule, or that *they* did; that they will

will not again pretend, that this ſeries is not formed *by the rule before us*, becauſe 1 *is neither* 4, *nor the ſquare of* 4, *nor any other power of* 4; or becauſe *no* particular *equation was ſuppoſed.* Since the rule contains in it this aſſertion, that 1 added to the ſum of the powers of 4 equals $-\frac{1}{3}$, and the abſurdity is not confined to a particular equation, but is found in any, which has one root equal to 4, and the reſt within the true limits of the problem.—The two remaining equations would ſhew us other abſurdities of the ſame kind, as that $-3+9-27+81$ $-$ &c. $=-\frac{3}{4}$, and that $2+2+4+8+$ &c. $=0$. But this matter has been traced far enough.

Perhaps I ventured too far, when I undertook not only to point out the Writer's miſtakes, but to ſhew alſo, in my third remark on this propoſition, how he fell into them. The doctrine of infinites, I ſaid, had perplexed many a ſcholar, and ſometimes their teacher too. The difficulties into which it had brought us in this propoſition, were evident; and there were other examples which appeared to be of the ſame kind. Thus when I found in a mere arithmetical computation, (for ſuch is the preceeding corollary, and the example to it) 4 put inſtead of the fraction $\frac{p-p^5}{1-p}$, p being 1; I doubted not but the Writer had come to this

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conclusion, by conceiving some of the common reasoning, concerning such infinite serieses, as are formed from the division of a quantity by a binomial; to be more general, than it really is. But though it appears, as I conjectured, that he has no objection to such a proof, yet he will not admit it; because he *found out the same thing by one plain finite division, or rather knew it to be true without seeking.* It may not become me to dispute against the Professor's instinctive knowledge; but for his proof by common division, I cannot persuade myself that he will abide by it. For notwithstanding that it is *the easiest sum, that can well be imagined,* I must take the liberty to desire him to explain to me an easier. The design of it is to shew, that 4 is equal to 3. And no greater liberty is taken in this process, than his example has recommended to us. Let $x = 4$ and let $x = px - 3p + 3$. Then 4 being substituted for x, it appears that $p = 1$, or, since the Professor likes the expression better, *becomes* 1. Transpose px, and divide by $1 - p$, and you have $x = 3$, that is, $4 = 3$. Had x been put for any other number, the conclusion would have been exactly the same that it equals 3. And any quantity may be proved equal to any other, if we may divide them, as the Professor teaches us, by $1 - p$, when p is one.— But lest the ease of this example should disgust him, I will borrow another, which may perhaps be more agreeable to his palate. In a book of Algebra, I meet with these two equations

$$xxy - 9bby = 2bxx - 18b^3.$$

and $xxyy = 4bbxx + 4bbyy - 15b^4.$

Divide

Divide the firſt by $xx - 9bb$ and you have $y = 2b$. Subſtitute $2b$ for y in the ſecond, and you have this contradiction $16b^4 = 15b^4$. Are we then to conclude that the two equations are inconſiſtent? certainly they are not. But x being equal to $\pm 3b$, the quantity $xx - 9bb$, by which we divided the firſt equation, is nothing, and the concluſion $y = 2b$ erroneous. For tho' the rule may poſſibly not be found in the books of common Algebra, to which the Profeſſor ſo often refers me, yet it is an evident rule of common ſenſe, that if you multiply or divide, the quantities you would compare, by nothing, however complex you make the expreſſion which repreſents that nothing, your reaſoning is totally confounded, your concluſion, if you will give it that name, may be any thing you pleaſe.—When I wrote this obſervation, I was well aware, that the Profeſſor could defend his computation by his Algebra a little miſapplied. The objection in this place, was not that he neglected rules, but that he followed them blindly. That he might not miſtake my meaning, I produced for him a plauſible proof of his aſſertion. In this proof, which the Profeſſor conſiders as mine, but which was deſigned entirely for his uſe, the quotients of the quantities divided by $1 - 1$, (for he will not allow me to call it 0,) are infinite, and the abſurdity is from many eyes ſomewhat the more concealed. Perhaps he may now be inclined to accept my civility, and not ſo fond of ſhewing, that he *can prove the ſame thing by one plain finite diviſion.*

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Sometimes indeed a ſubtraction may have the form of a diviſion. If from a compound quantity conſiſting of ſuch affirmative and negative parts as are all together equal to o, you take another ſuch quantity twice, thrice, or *n* times, the remainder, if in appearance there be any, conſiſts alſo of affirmative and negative parts, and is all together equal to o. But the quotient of ſuch a diviſion, ſince the remainder is always o, may, with the ſame propriety, or the ſame impropriety be called 2, 3, *n*, or any thing elſe.

The Profeſſor having occaſion in his 51ſt page (a paſſage we muſt conſider preſently) to divide one ſuch quantity by another, where the expreſſion is general, puts it down as certain that the quotient $= 0$. In a particular example, he makes it 4 inſtead of o. The general determination is evidently inconſiſtent with the particular one. Each of the operations is a ſimple finite diviſion. It is not poſſible that both ſhould be right. Let us beg of the Profeſſor to inform us, on which of them we are to depend; and rather to give us ſuch reaſons, as will ſtand of themſelves, than to endeavour to ſupport them by great Authorities.

With theſe he conſtantly abounds; but I have hitherto paſſed them without notice; becauſe, as he does not always refer us to the particular paſſages, it is not eaſy to conjecture, how he has miſtaken them; and becauſe it is foreign to my purpoſe,

purpose, to enter into a dispute concerning either the meaning, or the merits of other books. But in his *Reply* to this objection he has fully shewn why he charges *his* error on Mr. *Mac Laurin*, Dr. *Saunderson*, Mr. *Demoivre* and Mr. *Nicholas Bernoulli*; and as their own words will be sufficient, I shall call upon them to vindicate themselves. He begins with Mr. *Mac Laurin*, who says, that *we are not to conclude, that the value of a fraction is equal to nothing, when the numerator and denominator of it vanish together.* Mr. *Mac Laurin* expresses himself accurately. If the numerator and denominator of a fraction are both variable and vanish together, the idea of its value is not lost, till they have vanished. The last ratio of them, *when they vanish*, is as much a quantity as any other ratio. And had the quotation extended only three lines farther, it would have been plain to every reader, that this ratio is the subject of Mr. *Mac Laurin's* observation. For he adds, *In such cases of the value of N* (the fraction) *is found by computing* $\frac{\dot{P}}{\dot{Q}}$; *because when P and Q decrease, till they vanish, the ultimate ratio of P to Q is that of* $\dot{P}$ *to* $\dot{Q}$. *If* $\dot{P}$ *and* $\dot{Q}$ *vanish at the same time, then* $N = \frac{\ddot{P}}{\ddot{Q}}$. — Dr. *Saunderson's* words are equally plain. *Let us first of all suppose*, says he, *the quantities* r *and* e *to be finite, but to pass in a finite time from something through all degrees of magnitude into nothing, so as to vanish both together; if then upon this supposition*

ſuppoſition we can diſcover the ultimate ratio of r *to* e, *we ſhall at the ſame time ſee the ultimate magnitude of the fraction* $\frac{r}{e}$, *and ſo we ſhall have all we want.*—Mr. *Bernoulli* too, as he is cited by Mr. *Demoivre,* gives a like account of the matter. He finds what he calls *the difference,* we commonly the fluxion, *of the numerator and the denominator.*—Every one of theſe Writers is careful to ſhew that he is ſpeaking only of the limit of the ratios of two variable quantities, ſuppoſed to decreaſe *in infinitum.* And had the Profeſſor applied their doctrine in a ſimilar manner, the objection to his ſolution would have been only this, that he introduces fluxions or ultimate ratios into a problem, which may be ſolved by the eaſieſt operations of arithmetic with a tenth part of the figures, and by moſt people, we may ſuppoſe, in an hundredth part of the time.

The Profeſſor is ſo well ſatisfied with his anſwer to the laſt objection, that he gives himſelf but little trouble with ſome that follow it. My obſervations on the third lemma were,

1. That it is very difficult to find the meaning of this problem.

2. That, when we have found it, the anſwer appears to be falſe in every poſſible caſe, unleſs we inſert exceptions not here mentioned.

3. That no demonſtration of it is attempted.

To all which the Author replies. *If I understand him right, the exceptions which he wanted are mentioned in my lemma. However, he does not say my lemma is false, or that I have misapplied it.* It is wonderful, if these exceptions are mentioned, that he did not transcribe the two or three words, which contain them. It is more wonderful, that the omission of these exceptions, which alters in all cases the quantity sought, should be supposed consistent with the truth of the rule: as if, when one asserts, that $A=B$; another, that $A=B-C$; there was no opposition between them.—

On the first and second problems I observed;

1. That, as the problems are proposed, the second is only part of the first; as they are answered, the first is a particular case contained in the second.

2. That the answer to the second is given by a series, of which every term consists of other serieses, squared, cubed, and then twisted together.

The reply to the first of these remarks is, that *it proceeds upon an error of the press*; that the Author had proposed here but one problem, *the solution of which was divided into two articles*; but that *the Printer*, omitting the figure 2 which marked it, *made the second article into a new problem.* Nothing is more likely than such a little omission. But let us not deny the honest Printer his

his due praiſe ; for his care to make the following parts of the chapter conſiſtent with the change he had introduced, by altering the 2d problem to the 3d, the 3d to the 4th, the 4th to the 5th ; and for his addition of a whole line, as a title to the 2d problem ; which, though utterly inconſiſtent with the *Author's* plan, was very neceſſary, when he had determined to divide it into two problems. *A little attention*, I think, could not *have diſcovered*, that ſuch a regular, continued change was made by the Printer. But as the Profeſſor has given us his word, let us accept it, and ſay no more on this ſubject.

The truth of my next remark could not be denied. Whether it be a fault, or not, cannot be demonſtrated. It is merely a matter of taſte. And whenever there is not demonſtration againſt him, I will have the pleaſure of ſubmitting to the Profeſſor.

The ſolution of the third problem, I obſerved, is right, when n is an even number ; wrong, when it is odd.—But the Profeſſor tells me, that *even and odd have nothing to do in the caſe*, and immediately ſets himſelf to ſupport this aſſertion by an example in which n is three. And then inſtead of changing the equation by the ſeries given in his book, he changes it by the rule, from which that ſeries ſhould have been formed ; and puts to every quantity $+$ for $-$ or $-$ for $+$. Having corrected the fault in this inſtance, and intending his *Reply* for the benefit of ſuch readers only, as will not give themſelves the trouble of

looking into the book; he ſuppoſes, that they *will be puzzled to find the Obſerver's difficulty*; and aſſiſts them with a conjecture, not a very lucky one, that I wanted to have the equation brought into that form, which his ſeries required and I had blamed. But the change of the ſigns he ſpeaks of as no *great* matter. Be it as little, as he pleaſes. Yet he ſhould conſider that in many caſes it is the *whole* matter, in which the ſolution of the problem conſiſts. Whenever the equation wants the ſecond term, (and the common rules teach us to take away that term) the problem is anſwered by only changing one or more of the ſigns.

Theſe were my remarks on his firſt chapter, not general, as he frequently complains, but each pointing out a particular fault in a particular paſſage. Upon a review of them, no one appears to have received a juſt anſwer. The chief deſign of them was to examine his abilities, as a reaſoner. But there were added three ſhort obſervations on his abilities, as a writer: that he wants art, wants perſpicuity, and wants attention. And though theſe obſervations were not confined to particular paſſages, they were not left without examples to ſupport them. Of the firſt defect I mentioned inſtances of various kinds, ſuch as, when any one ſhall read the book, will occur to him in almoſt every page.—Of the ſecond I gave a remarkable ſpecimen; the more remarkable, ſince the Author has not attempted to render it intelligible, though he has found a pen, that can expreſs his meaning.—To ſhew his want

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of attention, it was my buſineſs to produce ſome plain miſtakes in ſome eaſy proceſs. The more obvious the faults, and the more eaſy the correction of them, the better would be the proof of my aſſertion. I choſe therefore a common algebraic diviſion, and aſſerted, that, beſides many errors in the ſteps, the quotient was wrong, and the remainder wrong in *three* out of four terms. And theſe operations appeared to me ſo plain, as to leave no room for any diſpute. But what propoſition can be ſo plain? He aſſerts, that his remainder agrees with mine in EVERY TERM. Let us place them together. The true remainder is

$9x^4+12x^2+2x+6$. The remainder in his book

$$\begin{array}{llll} 4x^2+5x+6 & -\ 3x^2. & 3x^2 \\ & \quad\ x & \ \ x \\ & & \ \ 3 \end{array}$$

He tells us, that we muſt read the quantities placed under each other, as if they were connected by a *vinculum*; and adds, that though *this notation is not very common, it is not peculiar to* him, *and is ſufficiently analogous to the common notation to be very intelligible to a reader of any attention and experience in theſe ſtudies.* I much doubt, whether an example of this notation can be found in any book of ſuperior character to the *Lady's Diary*. But why does the Profeſſor imagine, that I did not read it, as he does himſelf? The truth is, that, when I had gueſſed at his meaning, and put the quantities together, as he gives them, his remainder appeared to be $-9x^4-4x^2+8x+6$ agreeing with the truth in

only

only one term $+6$. If we suppose the $-$ in the first line to belong also to x in the second, then the quantities properly reduced will be $-9x^4-6x^3-6x^2+2x+6$ and two of these five terms will be right. And if we allow another $-$ to be inserted, and another uncommon notation to be used, $-\sqrt{-3xx+x}$ instead of $+\overline{3xx-x}$, than all the difficulties concerning this remainder are removed.—It may be said perhaps that these faults are easily corrected. Very true; and so is the mistake in the quotient; and so are *twelve* other small mistakes (*not one blunder*, the Professor assures us) in the Process. And for that reason, they afforded the fullest proof of the matter, for which they were alleged, the Writer's want of attention.

Among all these censures, I found, and was pleased with finding, some place for praise. But the Professor intimates, that it is not due to *him*. *My substitutions*, says he, *the Observer supposes to be new*. Whether they are new, or not, I own myself ignorant. There are many pieces of *Algebra*, which I have never opened. All that I pretend to in this science is to be able to distinguish good reasoning from bad. And it may perhaps be thought some confirmation of these pretensions, if I distinguished what our Author had borrowed from his own.

CAMBRIDGE,
Mar. 3. 1760.